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CONCEPTUAL REFORM IN SCIENTIFIC REVOLUTIONS

ROBERTO TORRETTI

Univ. de Puerto Rico, Fac. de Humanidades, Rio Piedras, PR 00931, USA

'Αλλὰ χρόνωι ζητοῦτες ἐφευρίσκουσιν ἄμεινον

I shall speak about an aspect of scientific revolutions which, though duly noted by Thomas S. Kuhn in his famous essay (Kuhn 1962, p. 88; cf. Kuhn 1964), has not, in my view, been fully appreciated by him, nor by his critics and successors. For reasons that have to do partly with my own limitations, but also with the matter at hand, I shall restrict my comments to and draw my examples from major revolutions in fundamental physics, by which I mean historical processes that have brought about a change in the very concepts in terms of which the phenomena of motion and the states of physical systems are described. The aspect of these processes that I wish to bring to your attention is the role played in them by direct argumentative criticism of the concepts that are being transformed or replaced. I believe that such a discursive or "dialectical" criticism of concepts has contributed significantly in several cases to precipitate the development of a new conceptual system from the generally accepted one, and has provided good reasons for giving up the latter. Whether conceptual criticism has played a comparable role in other branches of science and in lesser revolutions is an interesting question which I shall leave open.

It is evident that such radical changes as I wish to consider here, involving the basic ingredients of the physicist's rational reconstruction of nature, cannot occur incessantly, or else the daily labors of scientific inquiry would lack a clear direction. However, it is a remarkable — and not yet wholly assimilated — fact of contemporary history that no less than two — and I would rather say three — major revolutions of this kind took place in the first three decades of the 20th century. (I refer to the advent of Special Relativity in 1905, General Relativity in 1915, and Quantum Mechanics in 1925.) The proximity of such events makes it very hard for us to believe that the goal of physics is the accurate representation of a ready-made transcendent truth; unless we are also willing to endorse the

sceptical conclusion that we can never tell how far we are from reaching that goal or how well we are progressing towards it. For if each revolutionary conceptual system of physics is liable to be swept away by the next one, we cannot even anticipate in what *terms* transcendent truth may be accurately represented. But the repetition of major scientific revolutions raises a difficult philosophical problem even for those of us who do not indulge in the fantasies of realism. Even if we are willing to appraise the advance of science purely from within, in the light of its own past and prospective development, it might seem impossible to draw a valid epistemic comparison between alternative conceptual systems and thus to ascertain the progress in knowledge brought about by a major scientific revolution. The reason for this seeming impossibility can be briefly stated as follows: Factual observation, which has hitherto been acknowledged as the court of last appeal for the settlement of scientific disputes, cannot be called upon to decide between two conceptual systems if these systems are involved in the very description of the facts observed; such, indeed, must be the case when the concepts in question include the basic categories of kinematics and other fundamental predicates of physical systems.

Immanuel Kant was probably the first philosopher who, to counter the onslaught of modern sensationism, uncompromisingly held that we need concepts and an intellectual framework even to have an experience. "Anschauungen ohne Begriffe sind blind" — he said — "sense awareness without concepts is blind". At any rate, we may remark, it is altogether dumb, for in order to be able to *say* what you are sensing you must sense it *as* something, i.e. you must, in Kant's words, subsume the particular intuition under a universal concept. Kant, however, did not run up against the difficulty I mentioned, because he believed that all concepts we might ever resort to for "spelling out sense appearances in order to read them as experience" must fall under a fixed set of "categories" of the human understanding, which moreover, in their application to the fixed "forms" of human sense awareness — namely, Euclidean space and Newtonian time — yield a set of "principles" — in effect, the quintessential assumptions of classical physics — to which Kant maintained we are invariably committed by the eternal nature of human reason. Thus, in Kant's view, conceptual change can never take place at the fundamental level at which we saw the aforementioned difficulty arise. The hard core of Newtonian kinematics and dynamics as expounded, say, in Kant's *Metaphysische Anfangsgründe der Naturwissenschaft*, was there to stay. The appropriateness of new scientific concepts and the validity of any new hypotheses involving them could always by judged in the light of experience ordered

by the "categories" in accordance with their attending "principles". As we all know, not one of the Kantian principles has survived the revolutions of early 20th century physics. Contemporary science is in no wise committed to Euclidean geometry and Newtonian chronometry, to the conservation of massive matter and instantaneous distant interaction, to strict causal determinism and the continuity of intensive quantities. One could, indeed, still vindicate Kant's approach by giving up the specifics of his categorial framework while retaining its more general, as yet unquestioned features. But such an attempt must raise at least two doubts: Does not the Kantian framework, when purged of its Newtonian features, become too abstract to be of much use by itself — that is, without any adventitious complements and qualifications — in the constitution of experience? And, if it is still sufficiently rich to be useful, what assurance is there that it will not be swept aside by a forthcoming conceptual revolution?

Anyway, this is not the time to dwell on a possible revival of Kantianism. In the context of the present paper Kant was to be remembered only for having first realized the function of the basic conceptual structure of experience and having prepared us, by his analyses, to grasp the dramatic significance of its mutability. One may indeed conjecture that the epistemic implications of radical conceptual change in physics were not appreciated sooner, directly in the wake of Relativity and the Quantum, due to a general mistrust of the Kantian approach caused by the overpowering influence of logical empiricism. (This conjecture probably holds, at any rate, for the academic establishment in the United States.) Writers of that philosophical persuasion — in particular, Hans Reichenbach — often cited Relativity as material proof that science owed its cognitive content to observation alone, and that the non-empirical framework of scientific description, far from being the manifestation of unchanging Reason, was freely agreed upon as a matter of convenience. The alleged duality of observed facts and stipulated conventions showed up during the final, maturer stage of logical empiricism in the notorious distinction between observation and theoretical terms of a scientific vocabulary, which served, among other purposes, to trivialize and thus effectively to sidestep the issue of conceptual change in physics. If a term is observational it must be possible, "under suitable circumstances, to decide by means of direct observation whether [it] does or does not apply to a given situation" (HEMPEL 1965, p. 178). Theoretical terms, on the other hand, are those that do not meet this requirement. A theoretical term obtains its full physical meaning by "partial interpretation" in the observational vocabulary, i.e. by the stipulation that certain sentences in which it occurs are true if and only

if certain other sentences in which none but observational terms occur are true. Theoretical terms were naturally supposed to include such expressions as *rest mass, proper time, spacetime curvature, state vector,* which have been the harbingers of conceptual change in 20th century physics. Although, as far as I can tell, nobody has claimed that observational words are fixed forever in form or meaning, it was understood that they remain undisturbed by even the most drastic changes in the theoretical vocabulary. Indeed, why should anyone wish to modify the scope of terms that are furnished, as they stand, with their own infallible decision criteria? The permanence of the observational vocabulary in times of scientific revolution would then ensure, through the partial interpretation of the theoretical words, the possibility of comparing the statements of successive theories among themselves and with the facts of observation.

It is now generally agreed that such a division of scientific language into observational and theoretical terms is untenable. There can be no decidable empirical predicates, no set of terms under which phenomena, merely by being watched, obligingly classify themselves. Moreover, the supposition that the peculiar vocabulary of a physical theory obtains its meaning by "partial interpretation" in terms of such ordinary words as would normally pass for "observational" clashes with one of the characteristic tendencies of modern physical science. From its beginnings in the 17th century, its practitioners have been wary of common sense notions and common sense judgments, and have admitted ordinary usage as a welcome auxiliary for the description of their field of study only under the condition that it should ultimately submit to the jurisdiction and corrective control of scientific discourse, couched in the accepted artificial terminology. No shared set of "observational terms" can therefore bridge the gap between different systems of fundamental physical concepts.

Thus it is understandable that the same authors — namely Paul K. Feyerabend and Norwood Russell Hanson — who first fought the distinction between observational and theoretical scientific terms, should also have been the first to claim that the several basic conceptual systems of physics were mutually incomparable — or "incommensurable", as it became fashionable to say. There is a sense in which they were doubtless right, for such systems, in order to do their job, must be somehow self-contained and autonomous, in the manner of a Kantian categorial framework. And yet the recent history of physics, in spite of the great changes it has gone through, does not exhibit such deep chasms as the word "incommensurable" suggests. If Relativity and Quantum Theory were wholly disconnected, in their conceptual set-up, from Classical Mechanics

and Electrodynamics, why should physicists find it necessary to instruct their students in the latter in order that they gain access to the former? Note that it is primarily the *concepts* of the classical theories which must be mastered in order to make sense of their successors. In other words, the student is taught to analyse experimental situations in the manner of classical physics so that he may learn to see them in a different manner. The seemingly paradoxical mixture of continuity and discontinuity in the history of physics, the succession of independent, mutually exclusive intellectual systems that nevertheless coalesce to form a living unity, becomes comprehensible and even natural as soon as one considers that each conceptual revolution in modern physics has been carried out by men deeply at home in the manner of thinking they have eventually abandoned, that their innovation arose from their perplexities, that each new system, being born, so to speak, in the old and out of self-criticism by its supporters, does not only cancel but also preserves its predecessor, in a way that varies in each case and therefore merits careful study, but which anyhow explains the persistent use of the old mode of thought as a preparation for the new one. When internal criticism leads to the replacement of a conceptual system by another, the bond which is thereby established between them can also serve to join the second system, through the first, to the thought-patterns of everyday life, from which the successive intellectual frameworks of physics have become increasingly divorced. More significant perhaps from the perspective we have chosen is the fact that when a new mode of thought issues from conceptual reform the problem raised by its real or alleged incommensurability with its predecessor is automatically solved. For there can be no question of *choosing* between the old and the new if the very existence of the latter is predicated on a previous acknowledgement of the failings of the former. If the old becomes disqualified by the same exercise in self-criticism that finally gives rise to the new, a comparison between the rival systems is not really called forth — indeed, the birth of one of them is the other's death.

A neat example of theory dislogment through conceptual criticism can be found in the First Day of Galileo's *Dialogo sopra i due massimi sistemi del mondo.* As you well know, Aristotle's cosmology heavily depends on his doctrine about the natural motion of the elements. Being simple, elements must move simply, unless of course they are compelled by an external agent to move otherwise. Aristotle recognizes two kinds of simple local motion, corresponding to the two varieties of simple lines out of which all trajectories are compounded, namely the straight and the circular. Since the four known elements, earth, water, air and fire, move

naturally in straight lines to and from a particular point, Aristotle concludes that there must exist a fifth element that naturally moves in circles about that same point (*De Caelo*, I, ii–iii; in particular, 268b11ff., 269a2ff., 270b27ff.). This element is the material of which the heavens are made and the said point is therefore the center of the world. This is the ground for Aristotle's separation of celestial and terrestrial physics, and indeed, as Galileo's spokesman Salviati says, it is "the cornerstone, basis and foundation of the entire structure of the Aristotelian universe" (GALILEO, EN, 7, 42). Now, even if we grant the premises, Aristotle's conclusion does not follow, for, as Galileo's Sagredo is quick to note, "if straight motion is simple with the simplicity of the straight line, and if simple motion is natural, then it remains so when made in any direction whatever; to wit, upward, downward, backward, forward, to the right, to the left; and if any other way can be imagined, provided only that it is straight, it will be suitable for some simple natural body." (EN, 7, 40.) Similarly, any circular motion is simple, no matter what the center about which it turns. "In the physical universe (*nell'università della natura*) there can be a thousand circular motions, and consequently a thousand centers", defining "a thousand motions upward and downward" (EN, 7, 40). Salviati goes even further: "Straight motion being by nature infinite (because a straight line is infinite and indeterminate), it is impossible that anything should have by nature the principle of moving in a straight line; or, in other words, toward a place where it is impossible to arrive, there being no finite end. For nature, as Aristotle well says himself, never undertakes to do that which cannot be done". (EN, 7, 43.) Thus, "the most that can be said for straight motion is that is is assigned by nature to its bodies (and their parts) whenever these are to be found outside their proper places, arranged badly, and are therefore in need of being restored to their natural state by the shortest path" (EN, 7, 56); but in a well-arranged world only circular motion, about multiple centers, is the proper natural local motion of natural bodies. Although the Copernican physics that Galileo was reaching for was eventually founded on the primacy of straight, not circular, motion, the Aristotelian physics and cosmology could not survive the internal criticisms voiced by Sagredo and Salviati at these and other places of the *Dialogo*. For, as the latter remarks, "whenever defects are seen in the foundations, it is reasonable to doubt everything else that is built upon them" (EN, 7, 42). No wonder, then, that the publication of Galileo's book in 1632 had such a devastating effect on Aristotelianism.

Perhaps the clearest instance of conceptual criticism leading to a scientific revolution is Einstein's modification of the classical concept of

time in §1 of his paper "Zur Elektrodynamik bewegter Körper". To understand him properly we should bear in mind that the kinematics in which he was trained in the late 19th century was no longer that of Newton's *Principia*, supposedly based on the unapproachable notions of absolute time and space, but rather the revised critical version of it proposed by Carl Neumann in his inaugural lecture of 1869, "Ueber die Principien der Galilei–Newton'schen Theorie", and perfected in the 1880's by men like James Thomson and Ludwig Lange. Neumann and his followers developed the concept of an inertial frame of reference, which is Einstein's starting point. In fact, Lange's definition of an inertial frame — which, by the way, is equivalent to Thomson's — is much more approriate to Einstein's needs than the one that he himself, somewhat carelessly, gives. (As you will recall, Einstein characterizes his "ruhende System" as "ein Koordinatensystem ... in welchem die Newtonschen mechanischen Gleichungen gelten" [EINSTEIN 1905b, p. 892], a condition blatantly at odds with the subsequent development of his paper.) Lange defines an "inertial system" as a frame of reference in whose relative space three given free particles projected from a point in non-collinear directions move along straight lines. Following Neumann, Lange also defines an "inertial time scale", i.e. a time coordinate function adapted to such an inertial frame, as follows: A given free particle moving in the frame's space traverses equal distances in equal times (measured by the scale in question). Relatively to an inertial frame furnished with an inertial time scale, one can meaningfully assert the Principle of Inertia as an empirically testable law of nature: Any other free particle — besides those used as standards in the foregoing definitions — travels with constant velocity (unless it happens to be at rest in the frame). What apparently no one realized until Einstein made it obvious is that the Neumann–Lange definition of an inertial time scale is hopelessly ambiguous. If t is such a time coordinate function adapted to an inertial frame F, and x, y and z are Cartesian functions coordinates for the relative space of F, then any linear real-valued function $t' = at + bx + cy + dz + k$ is also an inertial time scale adapted to F. Einstein overcame this ambiguity with his famous definition of time by means of radar signals emitted from a source at rest in the chosen inertial frame. This yields a time coordinate function unique up to the choice of origin and unit: the Einstein time coordinate of the frame. Relatively to an inertial frame furnished with Einstein time one can meaningfully assert the Principle of the Constancy of the Velocity of Light as an empirically testable law of nature: Any light signal — besides those used as standards in the foregoing definition — travels with the same constant speed *in vacuo*, regardless of

the state of motion of its source. Einstein's Principle of Relativity says that the laws of physics take the same form when referred to any kinematic system consisting of Einstein time and Cartesian space coordinates adapted to an arbitrary inertial frame. The joint assertion of the Relativity and the Constancy of Light Velocity Principles entails that any two such kinematic coordinate systems are related to each other by a homogeneous or inhomogeneous Lorentz transformation. Of the many well-known revolutionary implications of this result I need mention only one: two Einstein time coordinate functions adapted to inertial frames in relative motion with respect to each other do not determine the same universal time order of events. This alone spells the downfall of Newtonian physics.

The example I have just sketched suggests a few remarks of a more general nature. In the first place, let me recall that, even though the ambiguity of the Neumann–Lange definition of an inertial time scale may look like a major conceptual shortcoming, it was of no practical consequence before the advent of fast particles and high-precision optics shortly before Einstein. For, as Eddington showed some sixty years ago, under the assumptions of Special Relativity the Einstein time coordinate of an inertial frame virtually agrees with that defined by the fairly obvious method of very slow clock transport over that frame (EDDINGTON 1924, p. 15), and two such time coordinates adapted to two inertial frames will not differ significantly over short distances if the frames move past each other at a speed much less than that of light. This may help us understand why Einstein's criticism came when it did. Generally speaking, even if a conceptual system of physics has hidden or obvious defects, physicists will not normally criticize them out of a craze for intellectual perfection, but only when a conceptual improvement is required by the praxis of research. For it is concepts *in use*, i.e. insofar as they are involved in the design and interpretation of experiments, that form the living tissue of physical thought.

In the second place, it is worth noting that the ambiguity of Neumann–Lange inertial time can be corrected, without giving up the substance of Newtonian theory, by denying that the speed of signal propagation has an upper bound. If there is no such an upper bound, then, under the remaining assumptions of Special Relativity, the time defined by infinitely slow clock transport over an inertial frame will be the same for all such frames. Coordinate systems that consist of this time coordinate and Cartesian space coordinates adapted to different inertial frames are mutually related by so-called Galilei transformations, that preserve the form of the Newtonian laws. It is now common, therefore, to include in

formal statements of Newtonian mechanics a postulate to the effect that there is no uppermost bound to signal velocities or that the symmetry group of nature is the Galilei group. Such postulates, indeed, did not occur to anyone before Einstein's work was published, and they somehow involve a reformulation of Newtonian mechanics within the relativistic mode of thought. As a matter of fact, such postulates are testable and thus provide a means of experimental comparison — subject, of course, to the categorial framework of Relativity — between Newtonian and relativistic laws. This illustrates a common effect of conceptual criticism, whereby the criticized theory is not immediately discarded, but corrected in a way that makes it "commensurable" with the theory that is meant to replace it. An even better illustration is provided by Elie Cartan's restatement of Newtonian gravitational theory as a theory of curved spacetime, in which the linear connection and hence the curvature depend on the distribution of matter, and freely falling test particles describe spacetime geodesics (Cartan 1923; cf. Havas 1964). In this theory, inertia and responsiveness to gravity are one and the same *de iure*, and not just *de facto*, as in Newton's original formulation. This corrects the main conceptual defect that Einstein found in the latter (see below). And of course, when Newton's theory of gravity is thus expressed in the chronogeometrical idiom of General Relativity, who would dare to suggest that it is "incommensurable" with Einstein's theory?

In the third place, I must emphasize that I do not claim that Einstein actually achieved his conception of Special Relativity through the exercise in conceptual criticism that he prefixed to his first presentation of it. To establish a link between a given mode of thought and its successor conceptual criticism need not play a role in the actual genesis of the latter. It may just as well be put forward after conceptual change has occurred, as a reason for accepting it. Indeed, in order to recover the rational continuity of the scientific tradition it is sufficient that we, its heirs and current bearers, are able to find appropriate critical arguments that bridge the gaps of conceptual revolutions; it is not necessary that those arguments should really have been made at the time the revolutions took place.

Conceptual considerations have also guided Einstein's thought along the way from Special to General Relativity. As Einstein himself told the 85th Naturforscherversammlung in Vienna in 1913, the gravitational phenomena known at the time did not warrant a modification of the extraordinarily successful Newtonian theory of gravity. What made a change imperative, at least in Einstein's eyes, was the clash between Newton's theory and Special Relativity. To set his quest for a new theory of

gravity upon a definite course, Einstein seized on a conceptual difficulty that afflicted Newton's theory from its inception, though nobody seems to have been worried by it until then. If we spell out the Newtonian gravitational force on a body using Newton's law of gravity on the one hand, and Newton's Second Law of Motion on the other, we obtain an equation in which the mass of the body occurs as a factor on both sides. This explains why, though the gravitational force on different bodies — as measured by a dynamometer — can vary greatly at a given location it exerts exactly the same accelerating effect on them all. What remains unexplained, however, is why the mass of a body possesses this twofold significance, as responsiveness to gravitational attraction or gravitational "charge", and as resistance to it or inertia. Indeed, if one reflects on how the Newtonian mass or "quantity of matter" of a falling body thus masks its own presence, by undoing on one side of the gravitational equation what it does on the other, one is reminded of the notorious Lorentz–Fitzgerald conjecture, according to which the motion of a solid body across the electromagnetic ether is masked by the effect of that very motion on the intermolecular forces that hold the body together. (This analogy may have inspired the curious association, in Einstein's 1913 Vienna lecture, of the Michelson–Morley attempt to measure the relative velocity of the earth and the ether, with Baron Eötvös' experiment confirming the equality of inertia and gravitational charge — EINSTEIN 1913, p. 1255.) The relativistic reform of received ideas about inertia made it seem probable that the twofold Newtonian mass concept would fall apart. Thus, Max Planck thought it very unlikely that thermic radiation in a void cavity surrounded by reflecting walls should have weight. But then — Planck concluded — as such thermic radiation "certainly possesses inertial mass ... the generally assumed identity of inertial and ponderable mass, confirmed hitherto by all experiments, is evidently destroyed" (PLANCK 1907, p. 544). Einstein, however, based his speculations on gravity on that very identity. Persuaded as he was that "science is fully justified in assigning ... a numerical equality only after this numerical equality is reduced to an equality of the real nature of the two concepts" at issue (EINSTEIN 1956, pp. 56f.), he set out to develop a theory of gravity in which the quantitative equation between ponderable and inertial mass was not just the idealized statement of an observed coincidence, as it had been for Newton, but flowed from their conceptual identity. For uniform gravitational fields the identity of gravity and inertia is assured by the Equivalence Principle that Einstein introduced in 1907. This principle extends to all physical laws the scope of Newton's Sixth Corollary to his Laws of Motion, in the same way as Einstein's

Relativity Principle of 1905 had extended the scope of Newton's Fifth Corollary. In its original formulation the Equivalence Principle postulated the perfect physical equivalence of a reference frame at rest in a uniform gravitational field of intensity $\mathbf{g}$, with a reference frame that moves relatively to an inertial frame with constant acceleration $-\mathbf{g}$. This in turn entailed that a frame falling freely in a uniform gravitational field behaves in all like an inertial frame. But of course in the real world gravitational fields are approximately uniform only within fairly short distances and durations. To establish the identity of inertia with gravity also when the latter reacts to the non-uniform fields of real life, Einstein resorted to the geometrical interpretation of Special Relativity proposed by Hermann Minkowski, for which, at first, he had shown little sympathy. Minkowski had proved that Special Relativity in effect treats the arena of physical becoming — or spacetime — as a 4-dimensional Riemannian manifold with flat indefinite metric, in which inertial particles describe geodesic worldlines. The Equivalence Principle implies then that a test particle falling freely in a uniform gravitational field also describes a geodesic of the flat Minkowski metric. Einstein's masterstroke was to postulate that *any* freely falling test particle follows a geodesic of a suitable metric characteristic — more exactly: constitutive — of the prevailing gravitational field. Such a metric is normally not flat, but if it is assumed that it has the same signature as the Minkowski metric, it follows at once that the latter approximates it tangentially at each spacetime point. This accounts for the local success of Special Relativity. The essential identity of gravity and inertia is now secured with full generality, for inertial motion is conceived simply as free fall at a great distance from gravitational sources or in the local limit while free fall is acknowledged as the genuine motion of matter left on its own (its "natural" motion, so to speak). The geometrical view of gravity also enabled Einstein to surmount what he eventually came to see as a serious conceptual difficulty in Special Relativity. In this theory, the spacetime metric is taken for granted as a physically ungrounded structure which nevertheless fixes the worldlines of inertial matter. The metric of General Relativity plays a similar role with respect to freely falling matter, but, as befits a gravitational field, it does not lack physical sources, but depends, through the field equations, on the spacetime distribution of matter.

Einstein's development of a geometrical theory of gravity made good use of Riemann's theory of manifolds, which provides on its own an excellent illustration of a very important type of conceptual criticism leading to conceptual reform. In his inaugural lecture of 1854, "Ueber die Hypothe-

sen, welche der Geometrie zugrunde liegen", Riemann took the received form of physical geometry to task for relying on too narrow a concept of space. He showed that Euclidean 3-space, which classical physics had unquestioningly adopted as its basic framework for the description of phenomena, is only a very special case of a vast family of structures, now known as differentiable manifolds. Even the much more restricted subfamily of Riemannian manifolds, in which a notion of curve-length is defined by means of a symmetric non-singular covariant tensor field of rank 2, furnished the mathematical physicist with a far richer range of choices than he had ever dreamt of. Riemann's criticism of standard geometry did not by itself bring about a conceptual revolution in physics, but it paved the way for it, by enabling men like Minkowski and Einstein to think freely yet strictly of alternatives to the established framework of scientific thought. Similar instances of liberating generalization are not infrequent in the history of mathematics. They provide much of the soil from which innovative physics draws its nourishment. They also supply — as in the case I mentioned of Cartan's restatement of Newtonian gravitation theory — a background against which the old becomes commensurable with the new.

The development of Special and General Relativity from 19th century physics is probably unexcelled as an example of radical conceptual innovation issuing from the past through conceptual criticism. In the history of quantum theories conceptual links are less clear. The difference is due in part, no doubt, to the fact that, while the advent of Relativity was dominated, at both its stages, by the exceptionally lucid thinking of a single man, quantum physics was brought about by several scientists who were not equally anxious for intellectual clarity and coherence, and who in the best of cases would only agree with each other — as Pauli once said of Heisenberg and himself — "as much as this is at all possible for two independently thinking persons" (Wolfgang Pauli to Hendrik Kramers, 27 July 1925; quoted by MEHRA and RECHENBERG 1982, vol. 3, p. 322). But this very fact makes the history of quantum physics all the more interesting for a study of rationality in scientific change. For rationality, which is certainly not to be had as the outcome of an algorithm for the vindication of beliefs, exists, if at all, as a collective achievement of men and women, and therefore must rest on strivings often at cross-purposes with one another. (Though here, indeed, in contrast with other harsher collective enterprises of man, differences must be settled *dià lógou*, i.e. through argumentative discourse.) A chapter of just this manyfaced life of reason is what we find, for instance, in the history of (non-relativistic) Quantum Mechanics, both at its birth through the seemingly opposed yet unwittingly convergent

efforts of Heisenberg and Schrödinger, and in the succession of its so-called interpretations. From the standpoint I have taken here one ought to try to see this chapter in its connection with those that preceded it in the evolution of physics. Indeed, I surmise that a clear grasp of that connection, based on a cogent "rational reconstruction" of the transition from the classical to the quantum-theoretic mode of thought, might enable us to attain at last a shared understanding of the quantum-mechanical concepts and conceptions and to dispel the uncertainties concerning their meaning and scope. As I noted earlier, such a reconstruction need not tell the story "wie es eigentlich gewesen", as it really happened. Nevertheless, it is not an easy task and may come across some insuperable difficulties. The necessary evidence has been gathered, critically ordered and elucidated by Max JAMMER (1966, 1974) and again, in greater detail, by Jagdish MEHRA and Helmut RECHENBERGER (1982). It shows that the said transition was far less perspicuous to the agents involved than Einstein's development of Relativity — which may help explain why the true purport of Quantum Mechanics has never ceased to be a disputed question.

There is not much more I can say on the subject at this time, but by recalling a few facts I may perhaps inject some blood into my rather abstract hints. In §1 of the paper in which he proposed the hypothesis of light quanta, EINSTEIN (1905a) proved that the black-body radiation formula entailed by classical electrodynamics and statistical mechanics — now known as the Rayleigh–Jeans Law — not only failed to agree with experiment, but was inherently absurd. According to that formula the energy density of radiation emitted by a black body within a small neighborhood $d\nu$ of a given frequency is proportional to ν^2, and therefore the energy density of black-body radiation at all frequencies exceeds every assignable quantity. This showed that classical physics stood in need of radical reform, but, as is often the case with such arguments from absurdity, it gave no hint as to what to do next. So Einstein turned for a lead to the black-body radiation law derived by PLANCK (1900) from dubious theoretical considerations but confirmed thereafter by all actual measurements. Since black-body radiation, as KIRCHHOFF (1860) had shown, does not depend on the nature of the radiating body, one was free to choose any working model of the latter. Planck assumed a black body consisting of a collection of harmonic oscillators vibrating at all conceivable frequencies. To derive his radiation law he postulated that the energy $U(\nu)$ of the oscillators vibrating at any particular frequency was not a continuous, infinitely divisible magnitude, but a discrete quantity, composed of an integral number of equal finite parts. Planck then proved from classical

principles that such parts or "energy elements" $e(\nu)$ depend linearly on the frequency. Symbolically: $U(\nu) = ne(\nu) = nh\nu$, where n is an integer. The proportionality factor h, with the dimension of action (energy $\times$ time), is of course Planck's constant. Einstein argued that Planck's "determination of elementary quanta is, to a certain extent, independent of the theory of 'black-body radiation' constructed by him" (EINSTEIN 1905a, §2) and was able to conclude that "monochromatic radiation of low density behaves ... as if it consisted of mutually independent energy quanta of magnitude h" (ibid., §6). The hypothesis of energy quanta was in the next few years fruitfully applied to several phenomena, notably the vexing anomaly of specific heats at low temperatures (EINSTEIN 1907, DEBYE 1912), and, with the publication of Bohr's paper "On the constitution of atoms and molecules" (1913), it became the mainstay of a quick-paced research programme on atomic structure and spectral lines. But for all its astounding experimental successes, the quantum hypothesis remained throughout this period of the so-called Old Quantum Theory (until 1925/26) a fortunate yet gratuitous guess. The Old Quantum Theory conceived the atom as a classical mechanical system that can exist in a number of different stationary states, subject to the *quantum condition* that, for each generalized coordinate q and conjugate momentum p, the integral $\int p \, dq$ over any closed curve (in phase space) is equal to an integral multiple of h. As a consequence of this, a transition from one stationary state to another could only take place instantaneously, in a mysteriously discontinuous fashion, as the atom emitted or absorbed a fixed amount of energy, characteristic of the transition in question. Such behavior was of course utterly incompatible with the classical mechanical concepts and principles employed in the characterization of the stationary states. Since the quantum condition yielded good predictions but the theory gave no reason that might make it understandable, one clung to it fiercely as to a magic incantation. Thus, when young Heisenberg resorted to half-integral multiples of h to account for the "anomalous" Zeeman effect, Pauli objected that one would then "soon have to introduce quarters and eighths as well, until finally the whole quantum theory would crumble to dust" (quoted by MEHRA and RECHENBERGER 1982, vol. 3, p. 30). In the end, even Bohr acknowledged that mixing classical mechanics with the new quantum ideas was in fact a "swindle" though one which "even if it might be a crime from a logical point of view", could still "be fruitful in tracing the secrets of nature in many situations". (Bohr to Pauli, 11 December 1924.) As difficulties piled up and it turned out that Bohr's methods would not even provide a satisfactory model of the helium atom, it became increasingly clear that a

wholly new scheme for the description and explanation of atomic processes had to be developed from first principles. Pauli expected that "not only the dynamical concept of force, but also the kinematic concept of motion of the classical theory [would] have to undergo profound modifications" (Pauli to Bohr, 12 December 1924). Heisenberg's revolutionary paper "Ueber quantentheoretischer Umdeutung kinematischer und mechanischer Beziehungen" (1925) answered to just such expectations. Born greeted it as "an attempt to account for the new facts not by a more or less artificial and forced adaptation of the old familiar concepts, but by creating a new, truly appropiate conceptual system" (BORN and JORDAN 1925, p. 858). Heisenberg began by recalling that the formal rules employed by the Old Quantum Theory for the calculation of observable quantities "may be seriously criticized on the ground that they contain, as essential ingredient, relations between quantities (such as, e.g., the position and period of revolution of the electron) which apparently cannot in principle be observed; so that those rules evidently lack a perspicuous physical foundation" (HEISENBERG 1925, p. 879). To overcome this objection to the theory's conceptual stock-in-trade Heisenberg proposed to build a new "quantum-theoretical mechanics, analogous to classical mechanics, in which only relations between observable quantities occur" (ibid.). The gist of Heisenberg's proposal — as elucidated and elaborated by BORN and JORDAN (1925) — lies in the substitution of a *matrix* of time-dependent complex-valued functions for each of the time-dependent real-valued functions (generalized coordinates and momenta) employed in the classical description of a mechanical system. From such matrices a "Hamiltonian matrix" characteristic of the system is constructed on the analogy of the classical Hamiltonian function, in accordance with the rules of matrix algebra and the new matrix differential calculus introduced by Born and Jordan. A quantum-mecanical system is then fully described by a suitable number of pairs of conjugate matrices Q_j, P_j, satisfying the commutation relations $Q_jP_k - P_kQ_j = (ih/2\pi)\delta_{jk}$, $Q_jQ_k - Q_kQ_j = P_jP_k - P_kP_j = 0$ (where δ_{jk} stands for the unit matrix if $j = k$, and for the zero matrix if $j \neq k$), and the system's Hamiltonian matrix H. The evolution of the system is governed by the matrix equations $\mathrm{d}Q_j/\mathrm{d}t = \partial H/\partial P_j$, $\mathrm{d}P_j/\mathrm{d}t = -\partial H/\partial Q_j$, which are of course formally identical to the canonical equations of classical mechanics. The postulated commutation relations introduce Planck's constant at a point where some proportionality factor is inevitable, but no "magic numbers" show up in the principles of the theory. Although the rationale of Heisenberg's move cannot be made clear without further considerations (such as are persuasively spelled out in §1 of

the mathematical appendix to HEISENBERG 1930), the foregoing sketch suffices to show how the new mechanics, while radically changing the kinematic concepts of the old, managed to preserve its dynamical laws. Hence, as P.A.M. DIRAC (1926, p. 642) was quick to note, "*all* the information supplied by the classical theory [could] be made use of in the new theory". An even stronger link with classical mechanics is apparent in the alternative response to the difficulties of the Old Quantum Theory, independently developed in 1926 by Erwin Schrödinger and subsequently known as Wave Mechanics. Schrödinger had a deep aversion to quantum discontinuity — "this damned quantum jumping", as he once called it in Bohr's seminar (quoted by JAMMER 1966, p. 324; see also SCHRÖDINGER 1952). He was proud to offer a new approach to atom mechanics, which replaced the usual quantum condition by a different requirement that made no mention of "whole numbers" — integers do eventually turn up in his theory, but "in the same natural way as in the case of the *node-numbers* of a vibrating string" (SCHRÖDINGER 1926a, p. 361). As is well known, Quantum Mechanics took its now familiar shape after Schrödinger proved that Heisenberg's scheme and his own, for all their overt discrepancy, shared the same underlying structure and were in effect mathematically equivalent. Schrödinger's proof was all the more surprising as — in his words — "the departure from classical mechanics in the two theories seems to occur in diametrically opposed directions". Thus, while Heisenberg's theory with its arrays of discrete quantities indexed by pairs of integers had been described by BORN and JORDAN (1925, p. 879) as a "true theory of a discontinuum", "wave mechanics shows just the reverse tendency"; it is a step from classical point-mechanics towards a *continuum-theory*. In place of a process described in terms of a finite number of dependent variables occurring in a finite number of total differential equations, we have a continuous *field-like* process in configuration space, which is governed by a single *partial* equation, derived from a [variational] principle of action". (SCHRÖDINGER 1926b, p. 734.) I cannot go further into this fascinating historical juncture. I do hope, however, that Schrödinger's choice of words will make clear to what an extent he saw himself as fulfilling, not destroying, the spirit of classical physics.

Summing up: My chief contention is that in order to perceive the rationality of radical conceptual changes in fundamental physics one must view them as episodes of an intellectual history, the history of physical thought. The intellectual nature of this history precludes thoughtless turnabouts: new modes of thought stem from the old through self-criticism prompted by its internal difficulties and inherent tendencies. This is not to

deny that the basic concepts of physics are, as Einstein said, "freie Erfindungen des menschlichen Geistes," free inventions of the human mind (EINSTEIN 1934, p. 180), so that there is no way of calculating what they will be like in the future. Though unpredictable, they must be grounded, or else they would not be "of the mind" (nor, properly speaking, "free"). But this is just what we find in the sources, at least within the tradition of modern mathematical physics: the authors of conceptual innovations have always or nearly always sought to motivate them carefully and to exhibit their provenance from established notions, by drawing apposite analogies, setting up correspondences, and even by retaining for the new ideas suggestive old names. The view of physics as a form of intellectual life, whose major turning-points are catalyzed by critical reflection, is not favored by the tendency, endemic among philosophers of science, to treat physical theories — in the loose, ordinary sense of the word — as informal expressions of the logical entities that such philosophers call "scientific theories" in a contrived special sense. Whether a "theory" be conceived, in the strict philosophical acceptance, as a set of propositions closed under deducibility, or, after the current fashion, as a definite Bourbaki-like structure surrounded by a vaguely characterized host of applications, such a "theory" or "theory core" is not an open-ended enterprise of thought, but a fixed, finished ideal object, with no signs of origin or seeds of change, to which history — invention, criticism, reform — can only supervene as an external accident. "Theories", in either philosophical sense, can stand beside each other like Egyptian pyramids, can have their several features outwardly set into some kind of correspondence, but cannot proceed from one another inwardly. The structuralistic, "non-statement" model of theories is indeed more fitting than the other one, insofar as it somehow makes allowance for evolution within so-called "theory nets" through the exercise of genuine scientific thought in the development of applications. But it does not seem to be of much help for understanding genetic relations between successive modes of thought embodied in distinct theories (in the ordinary sense). It is by actually rethinking the great intellectual systems of the physical world, not by boiling them down to marrowless bones, that one may come to see reason in their history.

Note

In his Salzburg lecture, Joseph Sneed announced an innovation in the structuralist view of theories. He pictured the whole of empirical knowledge at any given time as a system of conceptually heterogeneous theories, held together by so-called *links*. The entire approach

hinges on the adequacy and fruitfulness of this notion of an intertheoretic link, which I understand is further explicated in a joint paper by Balzer, Moulines and Sneed, to be included in this volume. I do not doubt that, if successful, their project should result in a momentous contribution to epistemology. I am afraid, however, that Sneed's new picture, which still is built exclusively from clear-cut, static, extensional set-theoretic predicates, is too oblivious of science's genesis and growth to be of much help in understanding its rational development.

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